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# British Tax Credit Simplification, the Intra-household Distribution of Income and Family Consumption



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## Non-technical summary

This paper explores the replacement of the Working Families Tax Credit (WFTC), an in-work benefit in the UK, with the child and working tax credits in 2003 and its effects on household spending patterns. Whilst the reform did little in terms of changing the value of awards or eligibility requirements, one interesting feature is that in couples the new CTC would be paid to the partner designated as the “carer of the children”, whereas the pre-reform WFTC was paid in with the “main-earners” wages. The reform is therefore associated with a change in the relative bargaining positions of the male and female partners. This paper explores whether the improved bargaining position of women could have led to changes in household spending patterns.

Previous evidence has documented a link between female bargaining power and increased household spending on child items. The existing causal evidence, however, derives from much early time periods or the setting of developing countries. This contrasts to the evidence presented here for the UK at the beginning of the 21<sup>st</sup> century. Moreover, recent theoretical literature has suggested the possibility that the gender targeting of benefits becomes ineffective at influencing household spending patterns as economies develop. This is an empirical question, which the evidence presented in this paper helps to inform.

To identify the effects of the reform, the paper exploits the fact that in-work tax credits targeted low but not middle income households. Middle income households therefore form a control group that is used to proxy the change that would have happened to the affected group of households in the absence of the reform. Specifically, we combine a difference-in-difference strategy with a pseudo maximum likelihood estimator, to overcome particular econometric issues.

A number of interesting results emerge. Firstly, it is found that the reform led to changes in household spending in a number of areas. This evidence is inconsistent with the proposition that the gender targeting of benefits becomes ineffective as economies develop. Moreover, we observe spending increases concentrated amongst a group of goods that are directly consumed by children or have a child development aspect, whereas spending decreases occur amongst goods that are exclusively consumed by adults. A further key result is evidence of an apparent trade-off between household public goods consumed by the whole family. Here, some of the strongest reform effects are found, and demonstrates that the effects of intra-household transfers can extend beyond a narrow group of child expenditures, which have received the most attention in the literature.

# British Tax Credit Simplification, the Intra-household Distribution of Income and Family Consumption\*

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## Abstract

The UK Government enacted simplification of its tax credit system in 2003. An interesting consequence of the reform is that tax credit payments were split between partners in couples, causing a rare wallet to purse transfer. This paper presents evidence on the effects of the reform on family spending, using quasi-likelihood techniques, for a sample of low income couples with children. In areas of child goods, evidence of important spending increases are found, whereas spending decreases are observed amongst goods that disproportionately benefit parents. A further key finding is an apparent trade-off between spending on public goods that are not exclusively consumed by children, but may nonetheless have a child development dimension. Results are contrasted to earlier findings from UK 1970's child benefit reforms. The effects are consistent with a non-cooperative bargaining framework, in which partners differ in their relative preference for different household public goods.

JEL Classification: C21, D12, H31, I38, J08, J16

Keywords: family expenditure, child well-being, welfare reform

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# 1 Introduction

In April 2003, the UK Government implemented radical reform of the UK benefit system with respect to child-related payments.<sup>1</sup> The existing benefits had evolved from numerous earlier reforms by previous governments, resulting in multiple benefits containing amounts in respect of children. The most widely received child benefits were in-work tax credits.<sup>2</sup> The objective of the reform was to simplify the existing system by bringing together all child-related payments into a single means-tested benefit named the Child Tax Credit (CTC). An interesting aspect of the introduction of the new tax credit is the exogenous change in the intra-household distribution of income that occurs for couples with children. The new CTC would be paid separately from the other benefits to the partner dedicated as the ‘carer of the children’. This is in contrast to the pre-reform system where the male partner could in general claim ownership of all benefits, including child-related benefits.

It is often argued that households will give greater emphasis to child related expenditures and investments when mothers control a greater share of a given household resources; although recent theoretical work has questioned this assumption (Doepke and Tertilt, 2011; Browning, Chiappori and Lechene, 2010). Whilst economists have had a long running interest in this matter, an absence of natural experiments and randomised trials has meant that the body of empirical work assessing such claims is limited, particularly from advanced industrialised economies.<sup>3</sup>

In the UK, Lundberg, Pollak and Wales (1997) (LPW) exploit a child benefit reform that took

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<sup>1</sup>Other radical elements of the reform include the extension of in-work tax credits to those without children and increases in support through tax credits for those over 50 years of age. These aspects of the reform, whilst interesting, are not the focus of this paper.

<sup>2</sup>The focus of the reform, and this paper, is on means-tested child benefits only. The most widely received universal benefit in the UK is Child Benefit, not the subject of this discussion.

<sup>3</sup>A closely related pool of causal evidence from the development literature has shown that children benefit when the bargaining position of women is improved. For South Africa, Duflo (2000) explores the effect of a female receiving a pension on child anthropometric outcomes. Bobonis (2009) and Attanasio and Lechene (2010) examine the PROGRESSA program that makes cash transfers to low income households in rural Mexico; Duflo and Udry (2004) exploit rainfall shocks in Cote d’Ivoire, increasing female crop income relative to male income. In contrast, Braido, Olinto and Perrone (2012) found no evidence of gender bias in intra-household allocations in Brazil. A number of other studies have examined the relationship between measures of the intra-household distribution of power and a variety of outcomes. These studies, however, do not consider the issue of paying welfare benefits directly to women. Moreover, they reflect correlations, not causal evidence. See for example: Schultz (1990) (Thailand), Thomas (1990) (Brazil), Thomas (1994) (US, Brazil, Ghana), Phipps and Burton (1998) (Canada).

place in the much earlier social setting of the 1970s. First-difference equations are estimated around the reform and shifts in expenditure away from male clothing and towards female and child clothing are observed. Ward-Batts (2008) (WB) extends the LPW analysis to a broader range of goods.<sup>4</sup> More recently, Gregg, Waldfogel and Washbrook (2006) study the introduction of a UK conditional cash transfer, the Working Families Tax Credit (WFTC), in 1999. WFTC transferred resources to low income working households with children through the pay check of the working partner. Evidence of expenditure increases, in areas which are important for child development, are found including children's clothes, fruit and vegetables, and books. However, these results do not allow predictions to be made about expenditure patterns had WFTC been paid directly to women. For the US, Rubalcava and Thomas (2000) find that the generosity of payments of Aid to Families with Dependent Children (AFDC) to single women with children, affects spending decisions in couple households.

Recent game theoretic contributions raise a number of questions on the effects of intra-household transfers, and thus on the 2003 tax credit reform (Doepke and Tertilt, 2011; Browning, Chiappori and Lechene, 2010).<sup>5</sup> Doepke and Tertilt (2011) consider a setting where a husband and wife make voluntary contributions to an infinite number of household public goods. The authors present both preference-based and constraint-based models. Economic constraints or preferences are assumed to differ by gender. For the constraint-based models a distinct empirical prediction is that, as societies become more gender-equal, benefit targeting becomes ineffective at influencing household spending patterns. That is, an income pooling result may emerge, in which household demands do not depend on the intra-household distribution of resources. A lack of natural or randomised experiments has hampered progress in testing such predictions in advanced economies.

This paper empirically examines the tax credit reform taking place in the UK in 2003 and its

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<sup>4</sup>The fraction of income devoted to toys, pocket money, restaurant and takeaway meals all increase, whilst a 'men's tobacco' category (consisting of cigars, pipe tobacco, and snuff products) sees a decrease. Hotchkiss (2005) returns to the LPW sample adding families without children as a control group. The relative post-reform change in the ratio of women's to men's clothing expenditure is statistically insignificant.

<sup>5</sup>For earlier game theoretic models see Manser and Brown (1980), McElroy and Horney (1981), and Lundberg and Pollak (1993). The collective approach of Chiappori (1988) and Chiappori (1992) generalises this class of models with the central axiom that the bargaining process is efficient.

impact on household spending patterns. Firstly, the paper documents the new reform. It then asks, in the light of the recent theoretical predictions described above, if the benefit simplification lead to changes in the composition of household spending patterns. Moreover, it asks if the reform could have benefited children, through the increased bargaining position of women. It, therefore, contributes new evidence from an advanced economy in a recent time period on the effects of targeting benefits to women. Whilst previous studies have examined the impact of in-work tax credits on a variety of labour and non-labour market outcomes, few have examined the effects on spending behaviour (a rare exception is Gregg, Waldfogel and Washbrook (2006) above). In contrast to the previous empirical literature on the intra-household distribution of power, this study offers the advantage that the reform provides a change in the intra-household distribution of power which is exogenous. The estimates presented, thus, have a causal interpretation.

Dahl and Lochner (2012) exploit the fact that US Earned Income Tax Credit (EITC) expansions in the late 80s and mid 90s favoured low and middle income families over higher income families.<sup>6</sup> A similar feature of the UK welfare system is exploited in the identification strategy, using the fact that low income households are more likely to be affected by the reforms, relative to middle income households. Difference-in-differences (DID) equations are estimated for the log of expenditure.<sup>7</sup> The commonly used log-level specification is motivated by a constant elasticity model. An often neglected issue in the estimation of the log-linearised models by ordinary least squares (OLS) is that, under heteroskedasticity, estimates of the elasticities will not only be inefficient but also biased. The issue is addressed, in the DID setting, by presenting estimates of the reform effect from a poisson pseudo maximum likelihood procedure, proposed by Santos-Silva and Tenreyro (2006) in the estimation of constant elasticity models for trade data.

Informed by the recent theoretical contributions described above, the analysis extends to a wide range of household expenditures, as opposed to a narrow group of child and parent goods, such as child and adult clothing.<sup>8</sup> To preview some of the main findings, an absence of a re-

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<sup>6</sup>Their outcome is child test scores and not expenditure.

<sup>7</sup>This is in contrast to the 1970s Child Benefit reforms which were universal. The LPW and WB estimations therefore do not include a control group.

<sup>8</sup>Additionally, results for the ownership of a number of household durables are included in appendix C.

form effect at the level of aggregate spending categories is found, however, this masks important within category spending changes some of which are not exclusively consumed by children or exclusively by adults. Firstly, this underlines the importance of looking at spending on a range of household public goods and not just a narrow range of adult and child items when considering the effects of intra-household transfers. Secondly, the modest effects presented, deriving from differences in treatment intensity, are important as they are inconsistent with income pooling models of family decision making and thus inform our wider understanding of the household decision making process as societies develop. Thirdly, whilst we observe spending increases on goods that are likely to benefit children (toys and games; musical instruments) and decreases in spending amongst goods that disproportionately benefit parents (gambling; maintenance payments), the strongest effects are found on a group of goods consumed by all household members. For example, we find positive effects for spending on the home but this is offset with reductions in spending on holidays. In so far as these goods are consumed by children as well as adults, the overall effect of the reform on child well-being is unclear. This points to caution when assuming that transferring resources to mothers will lead to unambiguous improvements in child welfare. Finally, we contrast the results to the LPW and WB studies of the child benefit reforms taking place three decades earlier. Explanations are put forward for the key similarities and differences.

The paper proceeds as follows. The next section documents the details of the reform. In section 3 the household decision making model of Doepke and Tertilt (2011) is outlined with exogenous tax credit income being added to the setup. Section 4 discuss the identification strategy, followed by a description of the data used in section 5. The empirical results are presented in section 6, section 7 discusses an alternative explanation for the observed spending patterns, and section 8 concludes commenting on the success of the reform and relates the findings to both the past evidence and the recent theoretical literature. Further details of robustness checks are included in appendix A.<sup>9</sup>

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<sup>9</sup>Full tables of robustness checks are available online at: <https://sites.google.com/site/paulfisherhomepage/home/research>



## 2 The Reform

This section documents, from the perspective of the analysis in this paper, the important features of the 2003 UK tax credit reform. It begins by describing the pre-April 2003 system and motivation for the benefit simplification. The pre-reform benefits are then contrasted to the new system, emphasizing the impact on the intra-household distribution of income in couples.

Prior to 2003, WFTC was the most widely received means-tested child benefit in the UK. WFTC topped up the income of low paid married or cohabiting couples with children and lone parents. Typical of such in-work benefits, its objectives were to improve the work incentives of this group and reduce child poverty (see Blundell, Brewer and Shepard (2005) for a discussion of the WFTC introduction). In November 2002, 1.38 million families, received a WFTC award of which 639 thousand were couples. The average weekly award was £80.79, compared to an average gross weekly income of £200.99 of the main earner in recipient couples (Inland Revenue (2002c)).

Maximum amounts available in WFTC in 2002 are set out in table D3 of the appendix.<sup>10</sup> All WFTC eligible families could include a basic credit worth £61.02 per week. Greater support was available for larger families through per child elements. For example, for each child aged under 16 an additional £26.90 was added to the families maximum tax credit. Importantly, alongside the per child amounts, a generous childcare rebate was available. This was worth up to a considerable £200 per week for couples in which both partners were in eligible employment. Additionally, families paying income tax could claim a small tax rebate known as the Children's Tax Credit (CHTC). This was valued at an annual £529 in 2002 and abolished in April 2003.

For households not meeting WFTC work requirements, child amounts were available in a distinct set of means-tested benefits. Low income families in non-working households, could claim equivalent child related support through Income Support (IS) and Income-based Job Seeker's

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<sup>10</sup>Eligibility for WFTC required one partner of a couple or a single parent to have worked at least 16 hours per week, having sufficiently low joint income with joint savings and capital not in excess of £8000. A families maximum WFTC is calculated as the sum of the qualifying elements in table D3. The maximum WFTC is then reduced by 55 pence for every one pound of joint net weekly earnings above a threshold of £94.50 per week (2002 rates).

Allowance (IJSA). In May 2002, some 201 thousand couples with children received amounts in respect of children in IS payments and 92 thousand families received IJSA (Inland Revenue, 2002a,b) <sup>11</sup>

The design of the pre-2003 benefit system typically meant that the male partner would be able to claim ownership of benefits in the event of a disagreement within a couple. WFTC would normally be paid in with wages of the partner working at least 16 hours per week<sup>12</sup>, unless couples requested for the payment to be made to the non-working partner where both partners would need to agree.<sup>13</sup> This suggests three categories of WFTC eligible couples<sup>14</sup>: couples where only the male partner works at least 16 hours per week, couples where only the female partner works at least 16 hours per week and couples where both partners work at least 16 hours per week. For the first group of couples, payment of WFTC would be through the wages of the male partner. The second small group of couples are atypical in that the benefit system gave the female partner ownership of WFTC. For example, this group is equal to only 3.5 percent of the estimation sample. The final group of couples chose which partner received payment, although the female partner cannot lay claim to a share of the WFTC income without the male's consent. Finally, couples receiving the remaining out-of-work benefits are similar to case 3, where they de-facto chose which partner received payment.

The reform was implemented in April 2003 and two new tax credits - the Working Tax Credit (WTC) and Child Tax Credit (CTC) - replaced the existing WFTC and child amounts in the other benefits.<sup>15</sup> The amounts available in WFTC were essentially split into the two new credits. The

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<sup>11</sup>This array of pre-reform benefits formed a complicated system with the set of out-of-work benefits being administered separately from WFTC, not by the Inland Revenue, but by the Department for Work and Pensions. The Department for Work and Pensions is the largest of the UK government departments and it is responsible for welfare and pension policy. The Inland Revenue is the government department responsible for tax collection.

<sup>12</sup>Employers, therefore, had full information over an employees tax credit entitlement. Azmat (2012) explores this feature of WFTC and its effect on wages.

<sup>13</sup>Note, that even if a couple were able to choose a particular partner to receive payment, the choice does not necessarily bear any relation to the underlying bargaining powers created by the benefit system. For example, if a couple has the ability to select the female to receive payment, it would only be with the male's consent that this occurred. This mirrors the case of a couple sharing a joint bank account, where equal access to a joint bank account does not imply equal power over spending decisions - which is determined by the relative bargaining positions. See, for example, Pollak (2011) or Woolley (2003).

<sup>14</sup>Self-employed workers were paid directly and are excluded from this discussion and the estimation sample.

<sup>15</sup>In terms of the timing of announcements, the UK budget of 2000 outlined the Government's plan to reform the existing benefits. A consultation document was published in July 2001 'New Tax Credits: Supporting Families,

central objective was to create a more efficient system, in terms of administration and reduce complexity from a claimants perspective. Per child amounts in WFTC were allocated to the new CTC and amounts in respect of employment were allocated to WTC (see appendix table D4). Furthermore, all existing means-tested amounts in respect of children in the UK benefit system would be subsumed in CTC. CTC therefore forms a single means tested payment consisting of the per child amounts previously in WFTC and all other amounts in respect of children previously in IS/IJSA/CHTC.<sup>16</sup> WTC includes amounts in respect of being in paid employment. Importantly, the employment requirements match those of WFTC; meaning the effect of the reform on work incentives is small (see figure 1 (discussed below)).

Interestingly, payments of the new tax credits were divided between the ‘main earner’ and the ‘carer of the children’. In order to emphasise the role of rewards for work, WTC followed the payment rules of WFTC (described above) and was paid through the employer with wages to the ‘main earner’ in the couple. CTC, on the other hand, was paid to the partner designated as the ‘main carer’, typically the mother. In this way, excluding the atypical set of couples where only the female partner is in at least 16 hour employment, the reform gives mothers an entitlement to claim a substantial share of tax credit income which they previously did not enjoy. The magnitude of this intra-household transfer in resources is reinforced with the payment of the substantial childcare tax credit being paid along with CTC.<sup>17</sup> In October 2003, 87 percent of CTC payments went to the female partner and the average annual CTC claim represented approximately 8 percent of the annual disposable income of the treated group of households in the estimation sample and hence under control of the female partner post-reform (Inland Revenue (2004)).

Figure 1 illustrates graphically some of the main features of the reform for a couple with

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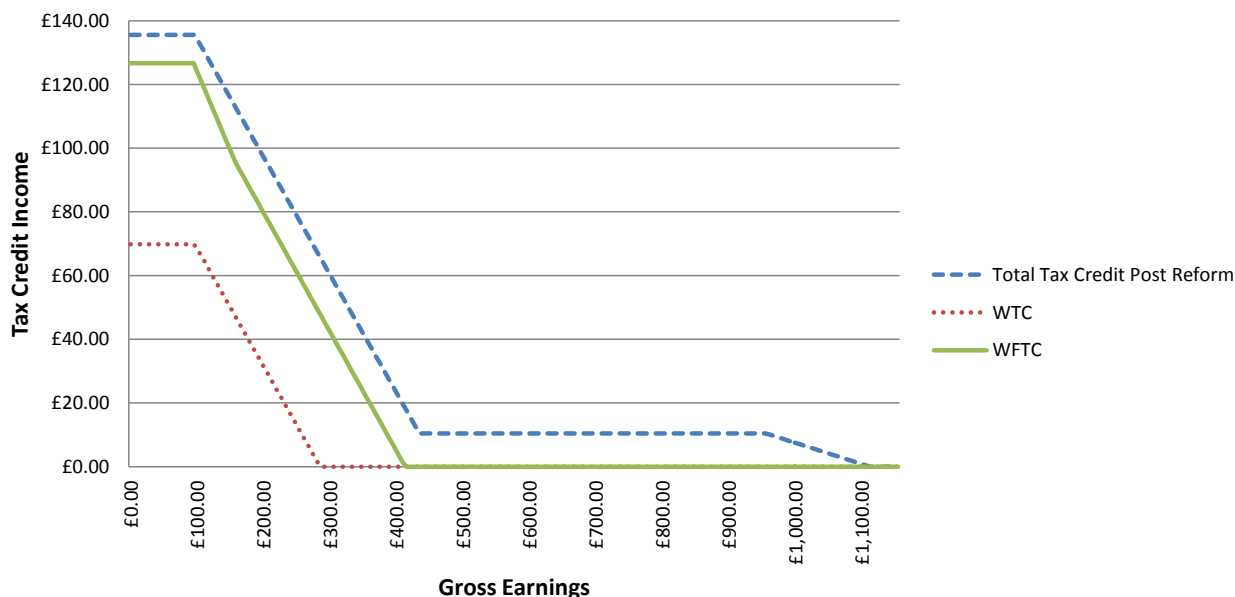
Making Work Pay and Tackling Poverty, Inland Revenue (2001). Following the consultation detailed plans were set out in ‘The Child and Working Tax Credits. The Modernisation of Britain’s Tax and Benefit System’, HM Treasury (2002).

<sup>16</sup>For some out-of-work families this implies small real terms increases. For example, a family with two children saw real term annual increases of over £460.

<sup>17</sup>The childcare tax credit was paid with CTC even though it forms part of WTC. The stated motivation was to acknowledge that the main carer usually organises childcare. 98,400 couples with children received support for childcare, which was now directed to the main carer (Inland Revenue (2004))

a single earner working 30 hours per week with 2 children. For a hypothetical family with zero earnings, the maximum tax credit under WFTC is slightly below the post-reform tax credit maximum, which is made up of its two constituent parts WTC (to the main earner) and CTC (to the main carer). CTC makes up the largest share of tax credit income post-reform. This results as the maximum amounts available in respect of children are typically greater than amounts in respect of paid work. Under the new tax credits, WTC payments (followed by the childcare rebate) would be withdrawn before CTC payments. Therefore, as household earnings rise, CTC constitutes a greater fraction of total tax credit income, and a greater share of tax credit income is under control of the female partner. A final feature to note is the long-tail of the new tax credit schedule. This occurs as the family element of CTC is not withdrawn until earnings reach a higher earnings threshold.<sup>18</sup>

Figure 1: Total Tax Credit Income for a couple with 2 children, husband working 30 hours per week



Source: Author's Calculations from applying the maximum tax credit amounts set out in tables 1 and 2.

Finally, on the transitional arrangements of the reform, for the main group of in-work claimants

<sup>18</sup>The family element of CTC approximately corresponds to the CHTC.

a relatively clear cut transition took place. Full details are included in appendix B.

### 3 Theoretical Framework

This section follows the household decision making model developed in Doepke and Tertilt (2011). Exogenous tax credit income is introduced to the setup in order to capture the UK policy setting and the main feature of the 2003 reform. The model is set in a non-cooperative bargaining environment. The non-cooperative setting is appealing as the underlying household decision making process is explicitly modeled.<sup>19</sup> A unique feature of the framework is the existence of a continuum of household public goods. Males and females differ only in their relative preference for the different public goods and this relative preference is related to gender.<sup>20</sup> This feature corresponds well to the data of the empirical results where expenditures are observed for a wide range of public goods, ranging from child and adult expenditures, to spending on goods enjoyed by all household members.

The model posits a male and female partner, each with independent labour and benefit incomes, who separately choose how to allocate resources between private goods and a continuum of household public goods. Consider the male and female preferences:

$$\log(c_f) + \int_0^1 i \log(C_i) di$$

$$\log(c_m) + \int_0^1 (1 - i) \log(C_i) di$$

where  $c_j$  is private good consumption of partner  $j$  ( $j$ =male,female),  $C_i$  is household public good  $i$  enjoyed by both partners ( $\int_0^1 \log(C_i) di$  represents consumption over the continuum of household public goods), and  $i$  is an index reflecting relative public good appreciation over the

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<sup>19</sup>One criticism leveled against the non-cooperative approach is the inefficiency that arises in public good provision, in a repeated setting, where it should be possible to avoid this inefficiency. Doepke and Tertilt (2011) show that introducing altruism to the framework leads to outcomes, which are close to efficient. The degree of inefficiency falls with the degree of altruism.

<sup>20</sup>To produce predictions of greater spending on child products would require that women have greater preference for these goods. This framework claims nothing about whether such differential preferences come from social norms or through inherent sex differences.

continuum of public goods. Female appreciation of public goods increases with the index  $i$ . The good with index  $i = 1$  is a female private good; the good with index  $i = 0$  a male private good; and goods with index  $0 < i < 1$  are public goods with varying strengths of male/female preference. For example, child goods would have an  $i$  close to 1 if they are more strongly preferred by females, where as, say gambling, would have an  $i$  close to 0 if it was a male preferred public good. Goods close to  $i = \frac{1}{2}$  are enjoyed similarly by both partners.

Contributions to public goods are perfect substitutes with  $C_i = C_{f,i} + C_{m,i}$ .  $C_{f,i}$  and  $C_{m,i}$  are female and male contributions to public good  $C_i$ , respectively. The individual budget constraints are:

$$c_f + \int_0^1 C_{fi} di = y_f + \theta_{TC}TC$$

$$c_m + \int_0^1 C_{mi} di = y_m + (1 - \theta_{TC})TC$$

with  $y_j$  the income of the partner  $j$  ( $j$ =male,female),  $TC$  the total tax credit the family receives (exogenous to the reform), and  $\theta_{TC}$  the proportion of tax credit that the wife can lay claim to. The reform of this paper exogenously increases  $\theta_{TC}$ .

The first order conditions for optimisation are:

$$C_i \geq i c_f$$

$$C_i \geq (1 - i) c_m$$

and hold with equality when a partner contributes to public good  $i$ . Therefore, male and female partners both make contributions to public good  $i$  only when  $\bar{i} c_f = (1 - \bar{i}) c_m$  for some  $\bar{i}$ . In equilibrium, public goods, for which  $i > \bar{i}$ , will be provided by the female partner and public goods, for which  $i < \bar{i}$ , will be provided by the male partner. We therefore have an equilibrium, where partners provide distinct sets of household public goods, with each partner providing the public goods they have the strongest preference for.

Following Doepke and Tertilt (2011), to show the existence of a unique equilibrium rearrange and substitute the male and female budget constraints into the cut-off condition  $\bar{i} c_f = (1 - \bar{i}) c_m$

giving:

$$\frac{\bar{i} 2[y_f + \theta_{TC}TC]}{3 - \bar{i}^2} = \frac{(1 - \bar{i}) 2[y_m + (1 - \theta_{TC})TC]}{2 + 2\bar{i} - \bar{i}^2}$$

The left hand side, which can be interpreted as the female's willingness to pay for the public good at the cut-off, is increasing in  $\bar{i}$  and is furthermore bounded between 0 (when  $\bar{i} = 0$ ) and  $y_f + \theta_{TC}TC$  (when  $\bar{i} = 1$ ). The right hand side, which can be interpreted as the male's willingness to pay for the public good at the cut-off, is decreasing in  $\bar{i}$  and is furthermore bounded between  $y_m + (1 - \theta_{TC})TC$  (when  $\bar{i} = 0$ ) and 0 (when  $\bar{i} = 1$ ). This implies a unique crossing point in  $[0, 1]$  and therefore a unique cut-off. The unique Nash Equilibrium can be summarised as:

$$C_i = \begin{cases} (1 - i) c_m & \text{for } i \in [0, \bar{i}], \\ i c_f & \text{for } i \in (\bar{i}, 1]. \end{cases}$$

We can now assess the impact of the reform on the cut-off and on expenditures on male and female preferred public goods. The reform transfers income away from the husbands wage packet and directs funds directly to the wife, thereby exogenously increasing  $\theta_{TC}$ . From the cut off condition with budget constraints, we can see that at the cutoff the wife's willingness to pay increases, while the husband's decreases. This implies that the cut-off  $\bar{i}$  must fall. Following the reform, the public goods the wife is responsible for expands, as she provides a greater range of public goods. Conversely, the range of public goods, the male partner is responsible for, declines.

Consider the expenditure  $F$ , on a group of public goods which are female produced both before and after the reform ( $a > \bar{i}$ ).

$$F = \int_a^1 C_{fi} di = \int_a^1 i c_f di = \frac{1}{2}(1 - a^2)c_f$$

as a fraction of total income this is:

$$\frac{\frac{1}{2}(1 - a^2)c_f}{y_f + \theta_{TC}TC + y_m + (1 - \theta_{TC})TC}$$

substituting the male and female budget constraints into the denominator gives:

$$\frac{\frac{1}{2}(1 - a^2)}{\left(\frac{3}{2} - \frac{1}{2}\bar{i}^2\right) + \left(1 + \bar{i} - \frac{1}{2}\bar{i}^2\right)\frac{\bar{i}}{1 - \bar{i}}}$$

This expression is decreasing in  $\bar{i}$ . Hence, as the reform increases  $\theta_{TC}$ , and  $\bar{i}$  shifts to the left, the fraction of income spent on these female produced public goods increases. Similarly, for a group of public goods M, produced by the male partner pre and post-reform ( $b < \tilde{i}$  where  $\tilde{i}$  is the post-reform cutoff).

$$M = \int_0^b C_{mi} di = \int_0^b (1 - i)c_m di = b\left(1 - \frac{1}{2}b\right)c_m$$

as a fraction of total income this is:

$$\frac{b\left(1 - \frac{1}{2}b\right)c_m}{y_f + \theta_{TC}TC + y_m + (1 - \theta_{TC})TC}$$

again substituting the male and female budget constraints into the denominator gives:

$$\frac{b\left(1 - \frac{1}{2}b\right)c_m}{\left(1 + \bar{i} - \frac{1}{2}\bar{i}^2\right) + \left(\frac{3}{2} - \frac{1}{2}\bar{i}^2\right)\frac{1 - \bar{i}}{\bar{i}}}$$

This expression is increasing in  $\bar{i}$ . Therefore whilst the expenditure share on the group of female preferred public goods increases, this is offset by reductions in the share of income devoted to the group of male preferred public goods.<sup>21</sup>

What can the empirical analysis draw from this framework? Firstly, the model predicts that the benefit simplification enacted in the UK in 2003 will impact on the composition of household spending. This is in contrast to other recent game theoretic models such as Browning, Chiappori and Lechene (2010), where income pooling results hold for some distributions of within household income. An important insight of the model is the trade-off in consumption that occurs between the male preferred public goods and the female preferred public goods. The model

<sup>21</sup>In this framework, overall spending on public goods is minimised when each partner has one half of income. See Doepke and Tertilt (2011).



raises the possibility that increased spending on child clothing say, could be offset by reductions in spending on food, for example. Of course, the question of which goods see spending increases and which see spending reductions, if any, is an empirical question. The empirical analysis, informed by this theory, gives attention not only to child goods, but to a wide range of household public goods, which may be important for child development.

A final point to note is on economic development and the effects of the gender targeting of benefits. Even in the absence of explicit forms of sex discrimination, such as labour market discrimination, the model predicts that the gender targeting of benefits will still be effective in changing household spending patterns, as long as there are preference differences across the sexes.

## 4 Methodology

This section details the empirical approach of the paper. We are interested to know if the composition of household expenditure changes as a result of the benefit reform introduced in 2003, which would furthermore inform our understanding of the particular model of household decision making in operation. The theory of the previous section warns that increases in child spending may come at the cost of investments in other important public goods. Informed by this theory, we wish to learn if the composition of household expenditure changes for a range of household public goods and not only narrow areas of child spending.

The impact of the reform is estimated using a DID strategy, adjusted for covariates. A pseudo maximum-likelihood estimator is exploited to overcome particular econometric issues arising from estimation of the linear model, as outlined below. The effect of the reform is given by:

$$E(e_1 - e_0 | T = 1) = E(e_1 | T = 1) - E(e_0 | T = 1) \quad (1)$$

Where  $e_1$  is a given household expenditure under a high degree of treatment intensity, and  $e_0$  is the household expenditure under a low degree of treatment intensity.  $T=1$  indicating that the

reform occurred. The problem is that only  $E(e_1 | T = 1)$  is observed under the reform. However, the difference in pre and post-reform outcomes of the treated group can be taken and a control group can be used to proxy the change that would have occurred in the outcome variable in the absence of the reform. Consider the case of one pre-reform period and one post-reform period, expenditure  $y$  on a given category for individual  $i$  is specified as:

$$y_i = \exp(\alpha + \beta_1 d_i + \beta_2 I(t > 2002) + \beta_3 d_i \cdot I(t > 2002)) \eta_i \quad (2)$$

where  $y_i$  measures weekly household expenditure,  $d$  is a treatment group indicator,  $I(t > 2002)$  a binary variable to indicate the post-reform period, and  $\eta_i$  a mean one error term. This specification imposes a constant percentage increase in household expenditure due to the reform, equal to  $e^{\beta_3} - 1 \approx \beta_3$ . Taking logarithms of (2) yields the DID equation:

$$\log(y_i) = \alpha + \beta_1 d_i + \beta_2 I(t > 2002) + \beta_3 d_i \cdot I(t > 2002) + \epsilon_i \quad (3)$$

where  $\epsilon_i = \log(\eta_i)$ . Interpreting (3) in a DID framework:  $\beta_1$  gives a constant mean difference between the treatment and control groups.  $\beta_2$  captures post-reform changes in the log of expenditure due to sources other than the reform, including prices, tastes/fashions, income, or general macro-economic effects. Crucially, such shocks are assumed to affect both the treatment and control groups equally, that is, the only factor affecting the relative log expenditures of the treatment and control groups should be the reform.<sup>22</sup> Assuming  $\epsilon_i$  is mean independent of the regressors, the

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<sup>22</sup>Section 7 explores the possibility of there being unequal income growth across the treatment and control groups. Other reforms in the estimation period affecting both the treatment and control groups are: the expansion of Free Nursery Education in April 2004; the Employment Act 2002 (EA); and Statutory Maternity Pay (SMP) reforms in April 2003. Since 1998 it was mandatory for all Local Education Authorities to provide free nursery places to 4 year olds for 12.5 hours per week. This was extended to all 3-4 year olds from April 2004. The EA conferred legal rights for employees to request flexible working patterns. Those with children under age 6 had the right to request flexible working with employers being legally required to take requests seriously. SMP is the minimum amount an employer is required to pay by law during maternity. Prior to April 2003, the law required that SMP was paid for a minimum of 18 weeks at a rate of pay of 90 percent of earnings for the first 6 weeks and then the minimum of £75 or 90 percent of pay for the remaining period. For mothers giving birth since 1st April 2003, the minimum increased to 26 weeks and the minimum amount increased to £100 following the first 6 weeks. In the event that these reforms changed household spending patterns, they would not pose a threat to identification, provided the treatment

change in the pre/post-reform expected expenditures is given by:  $E(\Delta \log(y)|d_i = 1) = \beta_2 + \beta_3$  for the treated group, for the control group  $E(\Delta \log(y)|d_i = 0) = \beta_2$ . Taking the difference in the differences removes the common time effect, leaving  $\beta_3$ .  $\beta_3$  therefore captures relative changes in the post-reform log-expenditure of the treated and control groups.

The logarithmic transformation of (2) raises two econometric issues in the estimation of (3) by OLS.<sup>23</sup> Firstly, observations, for which expenditure is zero, are dropped from the estimation sample. This sample selection represents a source of bias.<sup>24</sup> Secondly, to consistently estimate the parameters of (3) by OLS, we require  $E(\log \eta_i | x) = 0$ . Due to the log transformation of  $\eta_i$ , this holds only under restrictive conditions on  $\eta_i$ . Moreover,  $E(\log \eta_i | x)$  will depend on higher moments of the distribution of  $\eta_i$  and will therefore depend on the form of heteroskedasticity. If the variance of  $\eta_i$  depends on treatment status, then  $E(\log \eta_i | x)$  will be a function of the regressors, resulting in biased estimates of the reform effect. Santos-Silva and Tenreyro (2006) propose a poisson pseudo maximum-likelihood procedure to address the above, with an applied example to trade data. The authors' monte-carlo simulations suggest that, in the presence of heteroskedasticity, log-linearisation can lead to substantial bias in OLS estimates; however, their proposed procedure performs well under various forms of heteroskedasticity. Their procedure is applied in the empirical implementation of exponential DID equations.

The reform led to a wallet-to-purse transfer for couples receiving out-of-work benefits with child related components or in-work tax credits. Such benefits are targeted at low income families and are tapered away with income. This feature of the benefit system is exploited in the definition of treatment and control groups.<sup>25</sup> Households positioned at the bottom of the household earnings distribution are essentially treated by the reform and households in the middle of the

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and control groups were influenced equally.

<sup>23</sup>The issues were first identified in Santos-Silva and Tenreyro (2006) with reference to the case of gravity equations and trade data. Here, we have a DID setting.

<sup>24</sup>When examining specific items of expenditure, the problem of zero observations is potentially more severe; however, the magnitude of any bias depends on the characteristics of the data. For example, 62 percent of households report zero weekly expenditure on male clothing.

<sup>25</sup>Dahl and Lochner (2012) exploit the fact that US Earned Income Tax Credit expansions in the late 80s and mid 90s favoured low and middle income families over higher income families. Their outcome is child test scores and not expenditure. The LPW analysis of the 1970s child benefit reforms does not include a control group, due to the universal nature of the changes.

distribution, are affected to a much lower degree, but should be similar in expenditure trends and make a suitable control group. To determine the household income cut-off to define treatment and control status, an average family in the sample is defined. The family has a mean number of children, mean childcare costs and assumed to be qualifying for the 30 hour component of tax credits. Given that tax credit entitlement is decreasing in family earnings, a level of income is calculated, at which entitlement reaches zero for the average family.<sup>26</sup> This level of income forms the treatment/control group cut-off. Households at the top of the income distribution differ considerably from the treated group in terms of their expenditure patterns and are thus dropped from the sample.

A number of checks are implemented to confirm the robustness of the findings to the definition of the treatment and control groups, giving particular emphasise to the common trends assumption. A discussion of each of the robustness checks is included in appendix A. Footnotes comment on the sensitivity of the key results to the robustness checks.

While theoretically the labour market impact of the reform is small, in order to avoid concerns about the potential endogeneity of female earnings, the labour income of the male partner only is used in the construction of the treatment and control groups. Previous studies suggest that male labour supply is particularly insensitive to changes in financial incentives. Meghir and Phillips (2010), when reviewing labour supply in the UK, conclude that ‘male hours of work are almost completely unresponsive to changes in work incentives; however, male participation, particularly for those with low or medium levels of education can be responsive’.

Building on equation (2) allows for the inclusion of multiple pre and post-reform periods and will improve the precision of the estimates, where time effects are captured by year dummies and seasonal effects with month of interview dummies. Importantly, a rich set of household level

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<sup>26</sup>Note that the strategy identifies the effects of the reform from differences in the intensity of treatment. This results as the exact level of earnings at which a families WFTC entitlement reaches zero is family specific. Thus some families assigned to the control group may actually be receiving state support. For example, as tax credits are linked to family size, families larger than the average family remain eligible for positive amounts of tax credit above the zero entitlement level of the average family. Furthermore, higher earning families in the control group receive the children’s tax credit/the family element of CTC. See figure 1. For families with income near or above the zero cutoff of the average family, the amount of state support is typically small and thus any treatment effect of the reform is likely to be small. The exact threshold of the treatment cut-off is explored in robustness checks (see appendix A). An alternative strategy could construct family specific cut-offs but this would be selecting heavily on family size.

controls are added to the model. This leads to the model:

$$e_i = \exp\left(\alpha + \sum_{t=2002}^{t=2005} \beta_{1t}t + \sum_{m=feb}^{m=dec} \beta_{2m}m + \beta_3d_i + \beta_4d_i \cdot I(t > 2002) + \beta_5 \log(inc)_i + \beta_6 X_i\right) \eta_i \quad (4)$$

Where  $e_i$  is expenditure by family  $i$ ,  $t_t$  a set of year dummies,  $m_m$  a set of month of interview dummies to control for seasonal effects,  $d_i$  a treatment status indicator ( $d_i = 1$  for low income families),  $I(t > 2002)$  a binary variable to indicate the reform happened and  $X_i$  a vector of household characteristics.  $\beta_4$  is the coefficient of interest and gives the DID estimate of the percentage increase in spending due to the reform, which is adjusted for controls in  $X_i$ . A positive  $\beta_4$  indicates greater spending by low income households due to the reform.

The addition of  $X_i$  to the model allows us to control for sources of differences in treatment group specific trends which are observable. The expenditures under study are determined by a set of observable and unobservable characteristics, such as the number and age of children, age of the parents and education level, which may differ across the treatment and control groups. Differences in the levels of expenditure due to these characteristics is not a problem; but differences in trends would violate the DID identifying assumption of a time trend which is common to the treatment and control groups. The estimates turn out to be relatively unaffected by the inclusion of  $X_i$  to the model.

Equation (4) is estimated directly by PPML to overcome the sources of bias described above. Procedures to correct for the fact that regular standard errors may overstate the precision of estimates of a treatment effect in DID regressions have been the subject of much debate in recent years (see Bertrand et al., 2004; Donald and Lang, 2007; Wooldridge, 2003, 2006). If shocks are common to observations in a given group and year, then the error terms within a group are not independent but correlated. Moreover, in the case of multiple time periods, errors are also likely to be serially correlated. Here, we present heteroskedasticity robust standard errors. We justify the choice as: 1. sample sizes are relatively small and corrections will be conservative if observations are indeed independent, and 2. the set-up is similar to that of Meyer, Viscusi and Durbin

(1995), for which Wooldridge and Imbens (2009) comment that ‘It seems that, in this example, there is plenty of uncertainty in estimation, and one cannot obtain a tight estimate without a fairly large sample size.....it is hard to argue that the uncertainty associated with choosing low earners within the same state and time period as the control group somehow swamps the sampling error in the sample means’.<sup>27</sup>

Finally, the reform could lead to changes in both the levels and proportions of expenditure devoted to various expenditure categories. For this reason, alongside the preferred PPML estimates, results are also presented from a linear specification for expenditure shares (estimated by Ordinary Least Squares).<sup>28</sup>

## 5 Data

This section starts by describing the data set used. The characteristics of the treatment and control groups are examined, with attention also given to the compositional stability of the groups. Finally, the expenditure categories under study are discussed.

This paper takes advantage of expenditure data on couples by pooling the first five years of the Expenditure and Food Survey (EFS).<sup>29</sup> Beginning in 2001, the EFS results from the merging of the Family Expenditure and the National Food surveys. Primarily, the survey provides expenditure weights for the consumer and retail price indexes, national accounts estimates of household consumption expenditure and information for calculating nutritional trends. The EFS operates on an annual basis and is a continuous survey detailing information on approximately

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<sup>27</sup>Meyer, Viscusi and Durbin (1995) study the impact of increased benefit generosity on the length of time spent on a benefit compensation scheme. The authors perform analysis on 5626 individuals from one state with the treated group being high earners and the control group being low earners. The analysis is also repeated separately for the state of Michigan where an almost identical effect is found with only 1,524 observations with a t-statistic of 1.22.

<sup>28</sup>Here, the linear dif-in-dif model for expenditure shares has a simple interpretation with the common trends assumption referring to the expenditure shares. Given that expenditure shares are bounded between zero and one and are observed at the boundaries, means that specifying a linear model for the conditional mean of  $y$  may not be appropriate - the effect of a given variable on  $y$  cannot be constant. For this reason, the central results from the linear model were compared to estimates from a computationally more intensive quasi-likelihood approach (QML) proposed by Papke and Wooldridge (1996). The conditional mean of  $y$  is specified as the logistic function and then estimated using a Bernoulli quasi-likelihood estimator. The estimates line up with the OLS results and are available from the author on request.

<sup>29</sup>The EFS is managed by the Office for National Statistics. The data is available online through the Economic and Social Data Service. In 2008, the survey was replaced with the ‘Living Costs and Food Survey’.

seven thousand households throughout a given survey year.

Expenditure items for all individuals aged 16 or over in a household are recorded through a detailed expenditure diary.<sup>30</sup> The diary is kept over a two week period and all expenditure items in the period are recorded. Expenditures are then aggregated to the household level and into broad expenditure categories. The survey thus provides household level expenditure information for broad expenditure categories and disaggregated expenditures on specific consumption items. Above this, recorded in computerised household and individual questionnaires are information concerning household composition, regular purchases (eg. rent and mortgage payments), infrequent purchases (eg. fridges, televisions) and detailed individual level information including age, sex, region, income sources, education and marital status.

The estimation sample consists of single couple households (married or cohabiting) with at least one school-aged child aged 0-15 and responding to the EFS in one of the first five years (2001-2005) of the survey.<sup>31</sup> The EFS operates on the basis of a financial year (April-March) and the reform coincides with the start of the 2003 financial year. The sample is, therefore, made up of two pre-reform years of data and three post-reform years of data. The sample is further restricted to households, where both partners are between the ages of 24-50, non-sick or injured, non-self employed and not in full-time education.<sup>32</sup> EFS interviews took place across a year and all income and expenditure figures are expressed in December 2005 terms by applying the all items retail price index, available from the Office for National Statistics.

In line with the empirical strategy presented in section 4, tax credit entitlement is decreasing in family earnings and a level of income can be calculated at which entitlement reaches zero for the average family. For a family with characteristics at the sample means, tax credit entitlement reaches zero for weekly net earnings of £361.48.<sup>33</sup> This corresponds approximately to the bottom

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<sup>30</sup>A simplified diary kept by children aged 7-15 also features in the EFS.

<sup>31</sup>Households with children aged 16-18 were subject to Educational Maintenance Allowance reforms in 2004 and so are excluded from the analysis.

<sup>32</sup>Individuals aged over 50 were eligible for a separate labour market element of the reform, the income/expenditure relationship is noisy for those in full-time education and the self employed, and the disabled are likely to have expenditure patterns that differ from the rest of the sample.

<sup>33</sup>Applying the 2002 WFTC rules assuming: 1.84 children, £11.32 weekly eligible childcare costs and eligible for the 30 hour element of tax credits.

quartile of the household earnings distribution (25th percentile equals £374.03). As discussed in the previous section, to allay concerns that female labour supply could potentially be endogenous to the reform, treatment and control status are constructed using male earnings only. Constructing treatment and control groups using male earnings, rather than joint earnings of the couple, means that successful identification of the reform effect requires only male earnings to be exogenous to the reform.

Taking the distribution of male take home pay in a given year, the treated group is thus defined as households in the bottom quartile, whilst the control group is defined as households in the inter-quartile range in a given year. Couples reporting male normal take home pay in the top quartile of the normal male labour income distribution are likely to exhibit expenditure patterns dissimilar to the treated group and are thus dropped from the estimation sample. Imposing the complete set of exclusions leaves a baseline sample of 3,757 married or cohabiting couples with children, 1,257 in the treated group, 2,500 in the control group.

Table 1 presents the means and differences in the means for key control and labour market variables by treatment group status. Statistically significant differences are observed. Households in the low income treatment group tend to be younger (0.87 years for men, 1.38 for women), with less education (0.57 years for men, an insignificant difference for women), have larger families (0.12 extra children), less likely to be married (11 percentage points) and considerably more likely to be in social housing (20 percentage points). The differences in region of residence are generally small and statistically insignificant. The compositional stability of the treatment and control groups is also examined by comparing the pre and post reform sample means. Full details and a description of the results are included in appendix B.

The EFS aggregates expenditures into 13 major categories. Combining health with education and transport with communication leaves 11 broad expenditure categories: food and non-alcoholic beverages; alcohol and tobacco; clothing and footwear; housing, water and power; furnishing, household equipment and carpets; health and education; transport and communication; recreation; restaurants and hotels; miscellaneous goods and services; non-consumption (housing costs, fines, savings, holiday spending, home improvements). Evidence of reform effects in any



Table 1: Summary Statistics By Treatment and Control Group

<b>Controls</b>	<b>Low Income Treated<sup>2</sup></b>	<b>High Income Control<sup>3</sup></b>	<b>Difference</b>
<b>Age:</b>			
Male Partner	37.54	38.41	-0.87***
Female Partner	34.90	36.28	-1.38***
<b>Age Left Education:</b>			
Male Partner	16.82	17.40	-0.57***
Female Partner	17.33	17.54	-0.20
<b>Number of Children:</b>			
Total	1.93	1.80	0.12***
Age 0-4	0.62	0.60	0.02
Age 5-15	1.31	1.21	0.10**
<b>Housing and Marriage:</b>			
Social Housing	0.27	0.07	0.20***
Married	0.76	0.87	-0.11***
<b>Region:</b>			
North East	0.05	0.04	0.01
North West	0.10	0.10	0.00
Merseyside	0.02	0.02	0.00
Yorkshire and the Humber	0.09	0.08	0.01
East Midlands	0.07	0.08	-0.01
West Midlands	0.10	0.09	0.01
Eastern	0.07	0.09	-0.01
London	0.07	0.06	0.01
South East	0.09	0.14	-0.05***
South West	0.08	0.08	-0.01
Wales	0.07	0.06	0.02
Scotland	0.09	0.08	0.01
Northern Ireland	0.10	0.09	0.01
<b>Labour Market:</b>			
Household Income <sup>4,5</sup>	435.27	613.22	-177.95***
Employed(Male)	0.79	0.99	-0.21***
Employed(Female)	0.64	0.80	-0.17***
Work Hours(Male)	30.46	40.12	-9.66***
Work Hours(Female)	18.17	21.10	-2.93***
<b>Number of Households:</b>	1252	2505	

Notes:

1. Standard errors (robust) in parentheses. \* p<.10, \*\* p<.05, \*\*\* p<.01
2. Treated group formed of households in the bottom quartile of the year specific net male income distribution
3. Control group formed of households in the inter-quartile range of the year specific net male income distribution
4. £ per week (December 2005 prices)
5. Wages + investment income + social security benefits - taxes

one of the 11 spending categories would imply a rejection of income pooling.

Importantly, we also examine reform responses in a number of individual items of expenditure. Table 2 shows the pre-reform expenditure means for the specific expenditure items grouped according to goods consumed by: parents; children or with a child development dimension; and the whole family. The individual items are selected with reference to evidence from the household bargaining literature, where consumption of a good has been shown to be associated with a particular gender. This brings the benefit that some of the central results can be contrasted to the literature from different social and economic settings. Furthermore, we build on the existing literature by considering some new goods assignable to a particular spouse. For the adult goods, we follow Lundberg, Pollak and Wales (1997) who claim an ‘obvious choice’ is spending on male and female clothing<sup>34</sup>; ‘cosmetics’ which we conjecture are more extensively consumed by women in the household; ‘Takeaway meals’ which may represent a substitute for home production<sup>35</sup>; ‘gambling’ where, for example, Booth, Cardona-Sosa and Nolen (2014) in an experimental setting find that women are less likely to make risky choices than men; and ‘maintenance payments’ which, if children typically reside with mothers following divorce, would be paid in respect of the male partners children and reflect the male partners preference.<sup>36</sup> The child goods considered are: child clothing; fresh fruit and vegetables; childcare; books, newspapers and magazines; toys and games.<sup>37</sup> In line with the concerns raised by the earlier theory, we also contribute two new public goods that are likely to be consumed by the whole family: spending on the home and spending on holidays.

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<sup>34</sup>This would be violated if men derive utility from their wives appearance, say. The same is the case with cosmetics expenditure.

<sup>35</sup>Ward-Batts (2008) and Phipps and Burton (1998) find that female empowerment leads to greater spending on meals not prepared in the home.

<sup>36</sup>Maintenance payments do, however, benefit children in the receiving household.

<sup>37</sup>For child clothing see (Lundberg, Pollak and Wales, 1997; Rubalcava and Thomas, 2000; Attanasio and Lechene, 2002; Gregg, Waldfogel and Washbrook, 2006; Ward-Batts, 2008; Bobonis, 2009; Rubalcava, Teruel and Thomas, 2009); for fresh fruit and vegetables see (Phipps and Burton, 1998; Duflo and Udry, 2004); for childcare see (Phipps and Burton, 1998); for books, newspapers and magazines see (Gregg, Waldfogel and Washbrook, 2006; Ward-Batts, 2008); for toys and games see (Rubalcava and Thomas, 2000; Gregg, Waldfogel and Washbrook, 2006; Ward-Batts, 2008).

Table 2: Summary Statistics By Treatment and Control Group

Pre-Reform Expenditure	Low Income Treated <sup>2</sup>				High Income Control <sup>3</sup>				
	Level		Share		Level		Share		
<b>Parent Goods</b>									
Women's Clothes	9.37	(18.60)	0.0191	(0.034)	12.66	(23.11)	0.0193	(0.030)	
Men's Clothes	6.01	(15.49)	0.0123	(0.030)	8.48	(19.71)	0.0129	(0.029)	
Cosmetics	7.25	(9.39)	0.0158	(0.018)	9.99	(10.89)	0.0165	(0.017)	
Takeaway Meals	5.70	(7.51)	0.0138	(0.019)	6.16	(7.57)	0.0108	(0.014)	
Gambling	2.99	(6.12)	0.0069	(0.014)	2.87	(5.67)	0.0052	(0.011)	
Maintenance Payments	1.91	(11.07)	0.0042	(0.023)	2.08	(12.14)	0.0032	(0.018)	
<b>Child Related Goods</b>									
Children's Clothing	8.70	(14.93)	0.0189	(0.029)	10.20	(14.38)	0.0174	(0.025)	
Fresh Fruit/Vegetables	4.18	(3.80)	0.0106	(0.012)	5.54	(4.29)	0.0099	(0.008)	
Childcare	4.23	(17.97)	0.0073	(0.029)	10.88	(39.68)	0.0146	(0.047)	
Books	4.13	(4.91)	0.0098	(0.012)	5.16	(6.64)	0.0089	(0.010)	
Toys	4.33	(10.73)	0.0091	(0.019)	6.45	(13.27)	0.0104	(0.022)	
Musical Instruments	0.12	(1.48)	0.0002	(0.003)	0.62	(6.56)	0.0009	(0.009)	
<b>General Household Goods</b>									
Holiday	3.34	(11.24)	0.0069	(0.022)	5.31	(14.78)	0.0084	(0.023)	
Home Improvements	1.31	(9.31)	0.0026	(0.018)	7.89	(96.79)	0.0059	(0.044)	
<b>Number of Households:</b>		527				1052			

Notes:

1. Standard deviations in parentheses

2. Levels expressed in £ per week (December 2005 prices)

3. Treated group formed of households in the bottom quartile of the year specific net male income distribution

4. Control group formed of households in the inter-quartile range of the year specific net male income distribution

## 6 Results

This section presents the results. Footnotes comment on the sensitivity of the main findings to six robustness checks. A description of the six robustness checks is provided in appendix A.<sup>38</sup> The discussion contrasts the findings to the early empirical literature and relates them to the recent theoretical work. Section 6.1 shows the findings for the broad spending categories; 6.2 for the goods consumed by parents and general household public goods; and finally 6.3 for the child related spending.

<sup>38</sup>Full tables of robustness checks are available online. See footnote 9.

## 6.1 Major Spending Categories

DID estimates of the reform effect are firstly presented for spending at an aggregated level. Table 3 gives the estimated reform effect on each of the 11 major spending categories for the treated group of low income households. According to the theoretical framework of section 3 and in contrast to other recent theoretical predictions (discussed above), the new tax credits are predicted to lead to compositional changes in household spending. Moreover, households are predicted to shift resources towards categories of public goods that are relatively more preferred by mothers than fathers. Column 1 of the table presents the preferred PPML results from equation (4) excluding demographic controls. Following the simplification of the benefit system in 2003, we see changes in the expenditure of the treated group of households relative to the control. Increases in spending are spread across a range of spending categories. Rises in expenditure are observed for: alcoholic beverages and tobacco; clothing and footwear; housing, water and power; health and education; transport and communication; recreation; restaurants and hotels; miscellaneous goods and services; and non-consumption. Reductions in spending are seen in two areas only: food and non-alcoholic beverages; and furnishing, household equipment and carpets. However, all estimated reform effects are statistically insignificant. Column 2 examines the robustness of the findings in column 1 to differences in treatment group specific trends due to observable characteristics. The inclusion of the household level controls to the model does not have a substantial effect on the estimated coefficients, supporting the early conclusions of no overall reform effect and consistent with models giving income pooling results in the economic setting of a modern developed economy.

We can also ask how the share of expenditure devoted to a given category changes for low income families, relative to the control group, following the introduction of CTC in April 2003. This is explored in columns 3-4, which present results from linear models for expenditure shares (equation (4) first without and then adding controls). In line with the evidence presented in the previous columns, we see changes in the expenditure shares which are small in magnitude. The effects for food and non-alcoholic drinks do, however, become statistically significant. The result

Table 3: Estimates of the Reform Effect on Broad Expenditure Categories

	Full Sample				High Treatment Intensity Sample			
	(1) PPML	(2) + controls	(3) Shares	(4) + controls	(5) PPML	(6) + controls	(7) Shares	(8) + controls
Dependent variable:								
Food & Non-alcoholic Drinks	-0.024 (0.031)	-0.019 (0.028)	<b>-0.009**</b> (0.005)	<b>-0.008*</b> (0.004)	-0.046 (0.033)	-0.038 (0.031)	<b>-0.012**</b> (0.005)	<b>-0.010*</b> (0.005)
Alcoholic Drinks & Tobacco	0.013 (0.089)	0.017 (0.088)	-0.000 (0.003)	-0.000 (0.003)	-0.040 (0.098)	-0.024 (0.096)	-0.001 (0.003)	-0.001 (0.003)
Clothing & Footwear	0.027 (0.090)	0.040 (0.088)	0.003 (0.004)	0.004 (0.004)	0.013 (0.095)	0.027 (0.095)	0.003 (0.005)	0.004 (0.005)
Housing, Water & Power	0.045 (0.080)	0.043 (0.078)	0.002 (0.006)	0.001 (0.006)	0.019 (0.075)	0.048 (0.071)	-0.001 (0.007)	0.002 (0.007)
Furnishings, HH Equipment, Carpets	-0.092 (0.121)	-0.085 (0.119)	-0.007 (0.006)	-0.007 (0.006)	-0.155 (0.131)	-0.149 (0.128)	-0.009 (0.006)	-0.009 (0.006)
Health & Education	0.023 (0.362)	0.041 (0.350)	<b>0.005*</b> (0.003)	0.004 (0.003)	0.014 (0.417)	0.045 (0.392)	<b>0.005*</b> (0.003)	0.005 (0.003)
Transport & Communication	0.091 (0.067)	0.092 (0.066)	0.003 (0.007)	0.003 (0.007)	0.058 (0.072)	0.048 (0.071)	0.004 (0.007)	0.003 (0.007)
Recreation	0.009 (0.083)	0.022 (0.080)	0.004 (0.006)	0.005 (0.006)	-0.022 (0.084)	-0.001 (0.081)	0.003 (0.006)	0.006 (0.006)
Restaurants & Hotels	0.003 (0.058)	0.020 (0.057)	0.000 (0.004)	0.002 (0.004)	0.006 (0.064)	0.023 (0.062)	0.001 (0.004)	0.003 (0.004)
Miscellaneous	0.121 (0.074)	0.102 (0.071)	0.003 (0.004)	0.001 (0.004)	0.124 (0.081)	0.082 (0.077)	0.003 (0.004)	0.001 (0.004)
Non-Consumption	0.103 (0.076)	0.099 (0.071)	-0.004 (0.008)	-0.006 (0.007)	0.126 (0.089)	0.091 (0.082)	0.002 (0.009)	-0.005 (0.008)
Observations	3757	3757	3757	3757	3509	3509	3509	3509

Notes: Standard errors (robust) in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Models include a full set of year and month of interview dummies, a disposable household income variable, a treatment status indicator and a post reform indicator x treatment status interaction (coefficient presented). Controls are: a full-set of region dummies; variables for the number of male and female children residing in the household in age categories: 0-1, 2-4, and 5-15; an indicator for residing in social housing; controls for parental characteristics: age, age squared, age cubed, and age left full-time education indicators (greater than age 16 and less than 21 years old; greater than 21 years old). The high treatment intensity sample removes the highest earning 20 percent of households from the treated group.

suggests a 0.9 percentage point reduction in the share of expenditure devoted to this category. The results with controls in column 4 gives robustness to the findings with little change in the point estimates and effects that are more precisely estimated in some cases. Interestingly, the negative effects for food spending shares found here mirror those of Rubalcava and Thomas (2000) for AFDC in the US. The authors explain the pattern in terms of reduced spending on food eaten out of the home. However, the food and non-alcoholic drinks category in this paper contains only food eaten at home.<sup>39</sup>

For the main estimation sample, the income cutoff separating the treatment and control groups is set at the level for which tax credit entitlement reaches zero, for the average family in the sample (see section 4). Families either side of the cut-off, therefore, are typically receiving small amounts or are ineligible for tax credits. In order to explore the effects of receiving a greater intensity of treatment, columns 5-8 remove the top earning fifth of the treated group from the estimation sample. The treated group is thus now formed of households in the bottom fifth of the net male income distribution. While this definition of the treated group focuses better on the most affected group of households, the associated cost is that the treated and control groups now differ more in terms of their observable and unobservable characteristics. Results in line with the main findings would, however, be reassuring.<sup>40</sup> For this restricted sample, for the preferred PPML results with controls we again see statistically insignificant reform effects. The magnitude of the effects is larger for this group of households that received a greater intensity of treatment, although the effects are less precisely estimated. In the models for expenditure shares, again the estimated effects are a little larger in magnitude relative to the main estimation sample with the negative effects found for food and non-alcoholic drinks remaining statistically significant.

In summary, the evidence from table 3 points to the reform having little effect on the composition of household spending of low income households at the level of the 11 major spending areas. Evidence is suggestive of reductions in the share of expenditure devoted to food and

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<sup>39</sup>AFDC was paid only to single women with children and so, the authors argue, provides an outside option for low-income woman with children contemplating separation from her partner.

<sup>40</sup>A similar robustness check is performed that removes the bottom earning households of the control group. See robustness check 2 of appendix A for details.

non-alcoholic beverages, although the effects are statistically insignificant in the preferred PPML results. The finding is in stark contrast to the earlier evidence on the effects of intra-household transfers in different social settings discussed above that have rejected income pooling. The results would be consistent with a model that predicts income pooling as economies develop. However, if preferences differ by gender for goods within the 11 major spending categories, then results estimated above may hide important within category reform effects. The next section looks within these categories at specific items of expenditure at a disaggregated level.

## **6.2 Adult and General Household Public Goods**

The model of Doepke and Tertilt (2011) in section 3 predicts increases in expenditure on goods with a high degree of female preference which are offset by reductions in spending on household public goods with a high degree of male preference. If child goods represent the strongest female preferred public goods, and goods consumed by the parents are relatively more preferred by males, then the model predicts increases in child spending, offset by reductions in areas of spending directly benefiting adults. The model, however, demonstrates that the overall reform effect on child well-being may not necessarily be positive if there are spending reductions on public goods consumed by the whole family and they are male preferred. In line with this theory, tables 4 and 5 present estimates of the reform effect for a range of goods consumed by: parents; all household members; and children or with a child development dimension.

Panel A of table 4 shows estimates for the six goods, which directly benefit the parents in the household, whereas panel B shows estimates for two general household public goods consumed by the whole family. As with table 4, the results in columns 1-2 correspond to estimates from PPML specification 4 imposing a constant percentage increase in spending due to the reform, whilst for the expenditure share estimates in columns 3-4 are from OLS models. Columns 4-8 repeat the estimates for the high treatment intensity sample.

Starting with the adult goods in panel A of the table, reassuringly we see coefficient estimates which are stable in sign across the specifications and estimation methods. The sign of the esti-

Table 4: Estimates of the Reform Effect on Adult and General Household Public Goods

Dependent variable:	Full Sample				High Treatment Intensity Sample			
	(1) PPML	(2) + controls	(3) Shares	(4) + controls	(5) PPML	(6) + controls	(7) Shares	(8) + controls
<b>A. Parent Goods</b>								
Women's Clothes	0.104 (0.130)	0.136 (0.128)	0.001 (0.002)	0.002 (0.002)	0.065 (0.137)	0.094 (0.135)	0.001 (0.002)	0.001 (0.002)
Men's Clothes	0.042 (0.173)	0.046 (0.171)	0.001 (0.002)	0.001 (0.002)	0.058 (0.194)	0.059 (0.190)	0.001 (0.002)	0.001 (0.002)
Cosmetics and related	-0.065 (0.086)	-0.041 (0.085)	-0.001 (0.001)	-0.001 (0.001)	-0.055 (0.097)	-0.031 (0.095)	-0.001 (0.001)	-0.001 (0.001)
Takeaway Meals	0.118 (0.088)	0.122 (0.087)	0.002 (0.001)	0.002 (0.001)	0.140 (0.094)	0.137 (0.093)	0.002 (0.001)	<b>0.002*</b> (0.001)
Gambling	<b>-0.515*</b> (0.285)	<b>-0.418**</b> (0.191)	-0.001 (0.001)	-0.001 (0.001)	-0.456 (0.298)	<b>-0.380*</b> (0.201)	-0.000 (0.001)	-0.000 (0.001)
Maintenance or Separation Payment	-0.576 (0.428)	-0.521 (0.417)	-0.002 (0.001)	-0.002 (0.001)	<b>-0.937*</b> (0.499)	<b>-0.853*</b> (0.478)	<b>-0.003*</b> (0.002)	<b>-0.003*</b> (0.001)
<b>B. General Household Goods</b>								
Holiday	-0.301 (0.234)	-0.318 (0.224)	-0.003 (0.002)	-0.002 (0.002)	<b>-0.568**</b> (0.257)	<b>-0.555**</b> (0.243)	<b>-0.004**</b> (0.002)	<b>-0.003**</b> (0.002)
Home Improvements	<b>2.604***</b> (0.707)	<b>2.473***</b> (0.637)	<b>0.006**</b> (0.003)	<b>0.005**</b> (0.003)	<b>2.601***</b> (0.773)	<b>2.422***</b> (0.658)	<b>0.006**</b> (0.003)	<b>0.006*</b> (0.003)
Observations	3757	3757	3757	3757	3509	3509	3509	3509

Notes: see table 4 notes.

\* p&lt;.10, \*\* p&lt;.05, \*\*\* p&lt;.01. Standard errors (robust) in parentheses.

mated reform effects, whilst stable for a given expenditure category, varies across the categories. Evidence of important reform effects is found amongst the parent goods. For gambling payments, the preferred PPML estimates in column 1 show a statistically significant reduction in gambling expenditure due to the reform. The effect becomes more precisely estimated when the household level controls are added to the model and implies a reduction in spending due to the reform of -34.2 percent, calculated as  $\exp(\hat{\beta}_3) - 1$ . A statistically significant effect of a similar magnitude is found for the model with controls in the smaller high treatment intensity sample, although the effects for expenditure shares, while of the same sign, are never statistically significant.

For maintenance payments, which reflect male spending if children reside with mothers following a separation, we see that the treated group reduces spending in this area, relative to the control, after the introduction of the new tax credits. Coefficients are consistently negative across the columns and become larger in absolute terms and statistically significant in columns 5-8 when focusing on the most intensely treated households. The findings are robust to the inclusion of the



control variables. The PPML results with controls imply a reform effect of -57.4 percent. In the key area of male preferred goods, there is thus evidence that low income households reduced their spending following the introduction of the new tax credits.<sup>41</sup>

The central result presented in the LPW study of the 1970's child benefit reforms, was statistically significant increases in the ratio of women's to men's clothing. WB found similar results for the 1970's reforms. We are therefore particularly interested to see how adult clothing expenditure changed following the reform at the start of the 21st century. For women's clothing, we see small positive but highly insignificant reform effects for both the preferred PPML results and models for the share of expenditure. Furthermore, when focusing on the most affected households, the PPML estimates become smaller in magnitude and remain highly insignificant. The effects for male clothing are also positive and highly insignificant. It is concluded that in this area of spending the new tax credits had little impact on spending patterns.<sup>42</sup> One explanation for the contrast with the earlier literature is the dramatic fall in the price of clothing seen in recent decades, potentially making decisions regarding clothing expenditure less contentious. For example, whilst approximately 12 percent of household expenditure was allocated to clothing and footwear expenditure in the LPW paper of the 1970's, only 5 percent of household expenditure was allocated to clothing and footwear in the sample period of this paper.

For cosmetics and related, small highly insignificant negative coefficients are seen across the columns. Moving to takeaway meals we see positive coefficients across the columns. Interestingly, the estimated signs for both cosmetics and takeaway meals are in line with the earlier child benefit results of Ward-Batts (2008). The estimates of the tax credit reform in this paper are, however, small and statistically indistinguishable from zero, excluding the results for expenditure shares for the high treatment intensity sample.<sup>43</sup> For takeaway meals, one explanation for

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<sup>41</sup>The preferred PPML effects for gambling remain negative in all robustness checks but are statistically significant only in the case of robustness checks 2 and 3. For maintenance payments, the statistically significant effects remain in robustness check 2 and for the shares in robustness check 3. In the remaining robustness checks, the effects become less precisely estimated and are statistically insignificant. For the placebo reform, across all columns and spending categories of table 4, the estimated effects are small and statistically insignificant.

<sup>42</sup>The estimated effects for clothing remain small and statistically insignificant in all robustness checks. An alternative explanation could be a lack of statistical power, although we are able to detect reform effects in a number of other spending areas that follow.

<sup>43</sup>In robustness checks 2, 5 and 6, for the preferred estimates in the high treatment intensity sample, the effects

the lack of significant effects could be the rise of cheaply available quick to prepare meals and pre-prepared vegetables. This has reduced the time inputs required in the production of home cooked meals, and has created a cheap alternative to takeaway meals.

One concern raised by the recent theoretical developments and the theory of section 3, is that while more resources are predicted to be devoted to the most strongly preferred female goods (such as child goods) following the introduction of CTC, offsetting spending reductions in male preferred public goods may still impose a child development cost, if they are goods which the whole family consumes. We extend the previous literature to consider two such goods in panel B of table 4, which all family members are likely to benefit from: an obvious choice is spending on the home and another spending on holidays.

For holiday spending in the main estimation sample, negative effects are found in the PPML models with and without controls. While the effects are statistically indistinguishable from zero in the main sample, they grow in absolute magnitude when focusing on the most highly treated households in columns 5-8 and become statistically significant. For example, the PPML results in column 5 imply a decrease in holiday spending due to the reform of -43.3 percent, which is statistically significant. The inclusion of controls makes little difference to the estimated coefficients and tends to improve the precision somewhat. The pattern for the spending shares reinforces the finding with negative and statistically significant effects in the restricted sample both without controls and when controls are added to the model.

For the final general household good, particularly strong effects are found. The PPML estimates in column 1 imply an increase in spending on home improvements of 125.2 percent. The estimate is robust to the inclusion of the household level controls in column 2, falling a little in magnitude, but remaining statistically significant at the 1 percent level. The PPML estimates in columns 5-6 give further support to the finding showing positive coefficients, with the preferred estimate implying a statistically significant effect of a similar magnitude. The results for the expenditure shares, give support to the finding where we see positive statistically significant reform

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for takeaway meals slightly increase in magnitude and become statistically significant. The effects for cosmetics are always small and statistically insignificant.

effects in both samples and whether or not controls are included in the model.<sup>44</sup>

In summary, the evidence presented in table 4 has documented important changes in the resources low income households devote to specific items of expenditure, relative to their higher income counterparts, following the introduction of the new tax credits. In terms of adult preferred public goods, we see evidence of reductions in expenditure on gambling and maintenance payments, but interestingly in contrast to earlier reforms no effect for adult clothing expenditures. Importantly, the new evidence presented in this section suggests a trade-off in expenditure between the goods that are consumed by the whole family - increases are observed for spending on home improvements but reductions are seen in holiday spending. This result matches closely with recent insights from the theoretical literature.

### **6.3 Child Related Spending**

We are particularly interested to know how the new tax credits may affect child development and child related spending. It is not clear from the existing evidence, whether the new tax credits are likely to be effective in this regard. As described above, evidence indicates that following the 1970s child benefit changes, households devoted a greater share of resources to children's clothing. However, the period is characterised by an economic setting when, for example, female labour market participation was much lower than at the start of the 21st century. Whilst the findings from the development literature indicate that the gender targeting of benefits can indeed be effective, the available evidence is from countries in which the institutions and economic backdrop is quite different from that of the UK.<sup>45</sup> It is not clear from this evidence, which economic mechanism underpins results and thus, whether the gender targeting of benefits, in terms of child spending, is likely to be effective in the contemporary UK setting. The results of this section can therefore further inform this discussion.

Table 5 repeats the empirical exercise of the previous sections for the child spending items.

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<sup>44</sup>Strong positive and statistically significant reform effects are found for home improvement spending across all robustness checks (excluding the placebo reform where effects are small and insignificant). For holiday spending, negative and statistically significant effects are found in all robustness checks for the high treatment intensity sample (excluding the placebo reform where effects are small and insignificant).

<sup>45</sup>For example, strong gender norms may be present.

Table 5: Estimates of the Reform Effect on Child Expenditures

Dependent variable:	Full Sample				High Treatment Intensity Sample			
	(1) PPML	(2) + controls	(3) Shares	(4) + controls	(5) PPML	(6) + controls	(7) Shares	(8) + controls
<b>Child Goods</b>								
Children's Clothing	-0.066 (0.109)	-0.052 (0.107)	0.001 (0.002)	0.001 (0.002)	-0.048 (0.119)	-0.023 (0.116)	0.001 (0.002)	0.001 (0.002)
Fresh Fruit/Vegetables	0.079 (0.060)	0.062 (0.055)	0.000 (0.001)	0.000 (0.001)	0.053 (0.066)	0.036 (0.060)	0.000 (0.001)	0.000 (0.001)
Childcare	0.166 (0.305)	0.068 (0.297)	0.001 (0.003)	0.000 (0.003)	0.117 (0.355)	-0.002 (0.349)	0.000 (0.003)	-0.001 (0.003)
Books, Newspapers, Magazines	-0.036 (0.088)	-0.024 (0.085)	-0.000 (0.001)	-0.000 (0.001)	-0.040 (0.097)	-0.024 (0.093)	-0.000 (0.001)	-0.000 (0.001)
Toys and Games etc.	<b>0.269*</b> (0.157)	<b>0.255*</b> (0.155)	<b>0.003*</b> (0.001)	<b>0.003*</b> (0.001)	<b>0.309*</b> (0.173)	0.278 (0.171)	<b>0.003*</b> (0.002)	<b>0.003*</b> (0.002)
Musical Instruments	<b>1.800**</b> (0.861)	<b>1.795**</b> (0.777)	<b>0.001*</b> (0.000)	<b>0.001*</b> (0.000)	<b>1.780**</b> (0.874)	<b>1.888**</b> (0.778)	<b>0.001*</b> (0.000)	<b>0.001**</b> (0.000)
Observations	3757	3757	3757	3757	3509	3509	3509	3509

Notes: see table 4 notes.

\* p<.10, \*\* p<.05, \*\*\* p<.01. Standard errors (robust) in parentheses.

The table indicates that following the reform, consistent with the bargaining model, low income households increased their expenditures relative to the control group in some areas of child related spending. Both the PPML results for the full sample and the models for expenditure shares imply an increase in weekly expenditure on toys and games that is statistically significant. The columns test the sensitivity of the main results to the inclusion of the controls and give further strength to the finding where the effects remain statistically significant. For the smaller high treatment intensity sample, the estimates are of a similar magnitude excluding the PPML with controls where the effects are slightly larger but less precisely estimated. As with some of the earlier results for the adult preferred goods, the finding for toys and games mirrors that of WB for the 1970s child benefit reforms, where positive spending increases on toys were found.<sup>46</sup>

A similar pattern becomes clear for spending on musical instruments. We see positive and

<sup>46</sup>In robustness checks all of the estimated reform effects for toys and games are positive. They are statistically significant across all columns in robustness checks 3, 4 and 6. In robustness check 2, the preferred PPML results with controls are of a similar magnitude but not statistically significant (estimates for the shares and PPML with out controls in robustness check 2 are however positive and statistically significant). In the smaller sample of robustness check 5, the estimated effects are of a similar magnitude but less precisely estimated and statistically insignificant.

significant coefficients across the columns of the table. For the high treatment intensity sample with controls the effect is 32.0 percent.<sup>47</sup>

To comment on the remaining items in the table. For fruit and vegetables, the preferred PPML estimates are positive, but never statistically significant. For the remaining items, the estimates are highly insignificant and not suggestive of any effect on household spending patterns. This includes the case of child clothing, and matches with the result of no reform effect on women's clothing from the previous section.<sup>48</sup>

In summary, this section has presented evidence that the introduction of CTC lead to low income households allocating greater resources to important areas of child spending. Despite the key difference for child clothing, the result is similar to the evidence from the child benefit reforms three decades earlier with positive spending increases found for spending on toys and games. We also find increases in spending on musical instruments. Whilst the results do not point to one particular theory of household decision making, they do suggest that the appropriate model should be flexible enough to be applied across the setting of different countries and time periods. This fact points to an underlying model, which must be robust to the changes over time in female labour force participation, say. The result, like the results of the previous section, would fit with the bargaining model of Doepke and Tertilt (2011) in section 3 in which underlying preferences differ according to sex.

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<sup>47</sup>The preferred PPML estimates with controls remain positive and statistically significant in robustness checks 2, 3, 5 and 6. In robustness check 4 the effect is statistically significant for the high treatment intensity sample only (for the other columns in robustness check 4, the effects are positive and statistically significant). For the placebo reform (robustness check 1), the reform effects are small and statistically insignificant across all areas of child spending.

<sup>48</sup>Estimated effects for child clothing expenditures remain small and highly insignificant across the robustness checks.

## **7 An Alternative Explanation for the Observed Spending Patterns**

### **7.1 Unequal Income Growth**

Cribb, Hood, Joyce and Phillips (2013) document income growth for households with children in the UK over the period 1996-2012. Figure D1 replicates figure 5.2 of their paper to show income changes over the period at each percentile point of the income distribution for parents with children. While all families saw positive income growth, it is shown not to be equal across the distribution with the 2nd and 5th quintiles seeing the strongest growth of 1.8 percent and 1.9 percent, respectively. The lowest decile saw strong income growth of 1.4 percent, where as deciles 6-7 saw the weakest income growth of approximately 1 percent over the period. Could the relatively strong income growth of low income households over this period be responsible for the observed spending patterns recorded above? Indeed, the results of Gregg, Waldfogel and Washbrook (2006) indicate that spending by low income households on items used by children are particularly sensitive to income changes. We point to three pieces of evidence against the hypothesis. Firstly, we allow for household income changes to affect spending patterns in a limited way by including as a control in regressions a household income variable. The main results are insensitive to the inclusion of the household income control. Secondly, in robustness checks we restrict the control group to include households in percentiles 25-50 (robustness check 4 of appendix A). Note from figure D1 that for percentiles 25-50, like the treated group, relatively strong income growth over the period was seen. The results from the robustness checks line up with the main findings of the paper (see footnotes 41-48 of the previous section), which we think is indicative that differences in income growth between the main treatment and control groups cannot explain the observed spending changes in this paper. Thirdly, we perform placebo reforms where the treated group is made up of households in the 2nd quartile of the income distribution and control group formed of the top half of the income distribution (robustness check 1 of appendix A). In that the second quartile is made up of a relatively low income group that

saw strong income growth over the period, an absence of significant effects from this placebo reform is interpreted as evidence against the hypothesis that unequal income growth across the treatment and control groups is responsible for the observed child spending increases. For all of the individual spending items bar one, the estimated effects are small and statistically insignificant and point against unequal income growth being responsible for the main results of the paper<sup>49</sup>.

## 8 Conclusions

In April 2003, the UK government completed extensive simplification of its benefit system, replacing the existing system of means-tested child payments and tax credits with two new tax credits: The Working Tax Credit and Child Tax Credit. The interesting feature of this simplification is the exogenous change in the intra-household distribution of income that occurs for low income households. This paper is the first to explore the impact of the reform on family spending decisions. Several interesting findings emerge, which give insights into the household decision making process.<sup>50</sup>

Evidence is found that the reform caused low income households to change the composition of their spending. The findings survive a number of robustness checks. Whilst the size of the intent-to-treat estimates are modest, they are nonetheless important because they imply a rejection of any underlying model of household decision making which implies an income pooling outcome in the setting of a modern economy. Specifically, we find that the reform caused spending increases in some areas important for children and child development (toys and games; musical instruments), and at the same time caused households to decrease their spending on some goods more likely to be consumed by adults in the household (gambling; maintenance payments). A further important finding is the trade-off that is implied between expenditures on different household public goods consumed by the whole family. For example, the evidence presented shows

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<sup>49</sup>The effect for fresh-fruit and vegetables without controls is significant at the 10 percent level, but the effect drops out when controls are added to the model.

<sup>50</sup>The UK Government is planning a further round of welfare simplification with the aim of a single 'Universal Credit' replacing six existing benefits including WTC/CTC by 2017. Whether the new reforms will be fully implemented remains to be seen as the initial trials have been beset with IT problems. In couples, the Universal Credit will be paid to a 'nominated person', not necessarily the mother.

that whilst the reform lead to spending increases on the home, it lead to decreases in spending on holidays. In so far as these are two goods which are enjoyed by the whole family and may have a child development element, this makes the overall impact of the reform on child wellbeing less clear. Moreover, it highlights the importance of looking beyond a narrow range of child goods when considering the effects of intrahousehold transfers on children.

LPW reprised in Ward-Batts (2008) consider child benefit reforms from the late 1970s causing a wallet to purse transfer. How do the results of this paper relate to the findings from the earlier time period? In the LPW study, the ratio of women's to mens and child to men's clothing, are both found to increase following the child benefit changes. We find no evidence of clothing expenditure increases for the reform of this paper. An explanation put forward for this observation is the dramatic fall in the price of clothing in recent decades, potentially making household decisions regarding clothing expenditure less contentious. One of the central findings of this paper is the positive effects on spending on toys and games. Ward-Batts (2008) also finds positive effects for expenditure on toys. In contrast to the estimates for takeaway meals, we do not find significant effects, however the signs of the estimated coefficients are in the same direction. One possibility put forward for the lack of significant effects for takeaway meals is the rise of cheaply available quick to prepare meals and pre-prepared vegetables. This has reduced the time inputs required in the production of home cooked meals, and has created a cheap alternative to takeaway meals.

Despite the differences described above, the picture emerging from this paper shows similarities to that of the child benefit reforms taking place three decades earlier. This fact points to an underlying model of household decision making which must deliver the feature of greater child expenditures, when income is placed in the hands of women and this must be robust to the observed changes over time in female labour force participation, say. The result fits with the bargaining model of Doepke and Tertilt (2011), in which underlying preferences differ according to sex.



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## Appendix A: Description of Robustness Checks<sup>9</sup>

**Robustness Check 1: Placebo Reform:** A concern that the main expenditure findings are driven by differences in unobservables across the male net earnings groups or due to chance is further addressed by performing a placebo reform. The treated group consists of families in the 2nd quartile of the male net earnings distribution, who are unaffected by the reform; families in the top half of the male net earnings distribution form the control group.<sup>51</sup> If differences in unobservables across the male net earnings distribution are driving the main results, then we would expect to see statistically significant treatment coefficients for this placebo reform. To comment on the main findings of this placebo reform, all of the estimated coefficients for the adult preferred and general household public goods are statistically insignificant, whereas for the child spending areas only one of the estimated reform effects is statistically significant at the 10 percent level (PPML without controls for 'fresh fruit and vegetables'), but the effect disappears when controls are added to the model. This is strong evidence against the hypothesis that the main results are driven by differences in trends across the control and treatment groups or are due to chance.

**Robustness Check 2: Less Treated Control Group:** The reform effect is identified from differences in the intensity of treatment. Tax-credit entitlement declines with household income. To better focus the control group on households unaffected by the reform, this robustness check removes the lowest income households from the control group. The control groups is defined as households with male earnings between percentiles 30 and 75. The associated cost is that, under this definition, the treatment and control groups differ more in terms of their characteristics.

**Robustness Check 3: Male Partner in at Least Eligible Employment:** A sample restriction is imposed, which attempts to remove households from the treated group, who do not experience a wallet-to-purse transfer. The sample is restricted to households, in which the male partner is in tax credit eligible employment - that is reports working at least 16 hours per week. In this

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<sup>51</sup>Typically in a DID setting, it would be informative to perform placebo reforms outside of the sample period for the main construction of treatment and control groups. However, changes to the EFS survey and numerous other reforms taking place outside the main estimation period (for example the introduction of the Working Families Tax Credit in 1999) mean that such tests are uninformative.

way, the treated group consists of households which meet the tax credit hours of work eligibility criteria and it is the male who is likely to be receiving tax credits through the pay packet. If the reform operates through the mechanism of the intra-household distribution of income, then we would expect the observed effects to be stronger following this sample restriction.

**Robustness Check 4: Low Income Control Group:** The inclusion of the control group to the regression models is intended to capture the effect of time varying unobservables on consumption behaviour, such as price changes. A particular concern is that the control group could experience a different trend in expenditures to the treated group. Whilst the previous results were shown to be robust to the addition of control variables to the model, a concern remains that the estimated reform effects could be confounding relative spending changes due to unobservable variables. This robustness check restricts the control group to consist of couples, which are more similar in characteristics to the treated group. Specifically, the control group is restricted to include couples only in the 2nd quartile of the male net labour income distribution. That is, the main estimation sample consists of couples in the bottom half of the male net labour income distribution, with those in the top half of this distribution forming the control group. The cost of this restriction is a loss of sample size and precision.

**Robustness Check 5: Excluding Observations Seven Months Prior to the Reform:** This check explores the robustness of the main findings to the transitional arrangements of the reform. Applicants to WFTC after August 2002, received payment as a direct benefit payment, until the implementation of the reform in April 2003 (see appendix B). To check the sensitivity of the results to this transition, observations in the seven months prior to the reform are dropped from the estimation sample.

**Robustness Check 6: Differential Linear Trends:** The main estimation sample includes 2 pre-reform and 3 post-reform years of data, raising the possibility of controlling for differential treatment/control trends, albeit in a restrictive manner. If the linear specification is the appropriate one, then inclusion of these terms should not affect the estimated reform effects.

## **Appendix B: Transitional Arrangements and Compositional Stability of the Treatment and Control Groups**

### **Transitional Arrangements of the Reform**

The transitional arrangements of the reform should be considered in two parts: the transition for in-work families from WFTC to the new tax credits; and the transition for families receiving support in respect of children in out-of work benefits to receiving support in CTC. For the first group a relatively clear cut transition took place. Prior to April 2003, existing WFTC claimants received invitations to apply to the new tax credits, either online or by post. Existing awards of WFTC were appropriately shortened or lengthened to coincide with the reform in April 2003. In the lead up to implementation, payment through the employer was gradually phased out and replaced with a direct benefit payment from the inland revenue. Applicants to WFTC before 27th August 2002 continued to receive WFTC through the pay check for 26 weeks. Claimants after this date received their WFTC as a direct benefit payment. WTC payments through the employer began for all claimants in April 2003. For the second group of out-of-work claimants, families who made a new claim since April 2003 did not receive child additions in IS/JSA but should claim CTC. However, due to problems with the IT system operating tax credits, families claiming IS/JSA since before April 2003 continued to receive amounts in respect of children through these benefits, unless applying to CTC. At the end of the sample period (2005-06) approximately one third of out-of-work families still received support for children through out-of-work benefits and not CTC.<sup>52</sup> However, as the estimation sample in this paper is largely made up of in-work families, the relative size of this group is small.

### **Compositional Stability of the Treatment and Control Groups**

To examine the compositional stability of the treatment and control groups, differences between the sample means in the pre and post-reform periods were calculated (see table D1). The composition of the groups would change if, for example, husbands increased their labour market

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<sup>52</sup>The value of the award was the same whether families claimed through out of work benefits or CTC. Families could not claim both.



participation following the reform. The differences in the pre and post-reform means are comforting. The majority of the variables show insignificant changes between the pre and post periods. In the cases of age and age left education, the treated groups sees small increases; however, the changes are mirrored in the control group, indicative that the common trends assumption is reasonable, at least in terms of these variables. Household income of both groups is also increasing in real terms over the period with the treatment group being £26.06 better off in the post-reform years, for example. Finally, the stability of female labour supply should be noted, which sees small and statistically insignificant changes in both groups. This observation is important for the bargaining interpretation of final regression results.<sup>53</sup>

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<sup>53</sup>DID equations are estimated for female employment and weekly hours of work. The estimated reform effects are highly insignificant. In the main DID specification with controls, the estimated employment effect is 0.019 with a standard error of 0.030; for weekly hours worked the estimated effect is -0.136 with a standard error of 0.112.

## Appendix C: Durable Ownership

This section documents changes in the patterns of durable ownership of the treated households. The durable items are purchased infrequently, so may not show in the estimates of weekly spending. We are concerned that the increases in child spending of the previous sections could be offset with reductions in the possession of durable goods. Whilst the durable goods are not exclusively consumed by children, some do have child wellbeing dimensions.

Table D2 presents estimates of the reform effect on the possession of 8 durable items: mobile phones, cars, DVD players, computers, internet access, tumble dryers, microwaves and dishwashers. Columns 1-2 present the marginal effects of the *treated*  $\times$  *post* interaction from probit models. Columns 3-4 repeat the analysis for the high treatment intensity sample. Following the benefit simplification in 2003, we see from column 1 that treated households increased their ownership of mobile phones, computers and cars; whilst reducing their possession of DVD players, internet access, tumble dryers and dishwashers. Excluding the estimates for tumble dryers, the coefficients are small in magnitude and statistically insignificant. The estimates for tumble dryers imply a statistically significant reform effect of an 9 percentage point reduction in the proportion of treated households owning a tumble dryer. Tumble dryers, whilst benefiting the whole household, can be thought of primarily as an effort saving device for parents, rather than an item directly benefiting children.

Column 2 examines the robustness of the findings in column 1 to differences in treatment group specific trends due to observable characteristics. A very similar pattern emerges with the inclusion of the household level controls not having a substantial effect on the estimated coefficients but increasing the precision slightly. This gives strength to the early conclusion of significant effects only for tumble dryer ownership, a device from which gains largely derive to parents.

The marginal effects for the restricted sample give very similar results to the OLS models. Negative effects are observed for mobile phones, DVD players, computers, internet access, microwave and dishwashers; positive effects are found for cars. However, the effects are never

statistically significant. For tumble dryers, the estimated effect supports the previous finding with a highly significant coefficient implying a 9 percentage point reduction in ownership due to the reform. This result is again robust to the inclusion of the household level controls in column 4.

In summary, this section has examined the impact of the reform on the ownership of some key durable goods, which are important indicators of household wellbeing. Some of the items have a child development aspect. A concern raised by the earlier theory, was that spending increases on the female preferred public goods could be offset with reductions in the ownership of male preferred durable goods, which nevertheless are important for child development. Of the durables likely to contribute to a child's learning and development such as computers, no evidence is found of significant reform effects. Significant reform effects (negative) are found only for the possession of tumble dryers - an effort saving device for parents in the household.

## Appendix D: Additional Tables and Figures

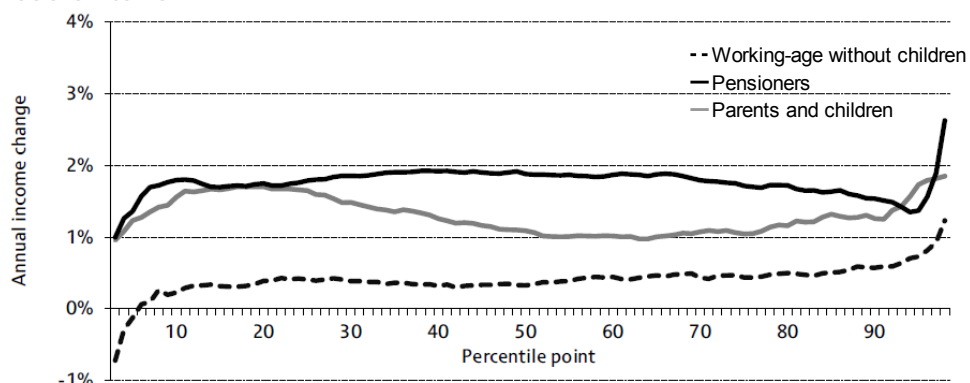
Table D1: Summary Statistics Pre and Post-reform

Controls	Low Income Treated <sup>2</sup>			High Income Control <sup>3</sup>		
	2001-02	2003-05	Diff	2001-02	2003-05	Diff
<b>Age:</b>						
Male Partner	37.33	37.69	<b>0.35</b>	37.95	38.74	<b>0.79**</b>
Female Partner	34.81	34.97	<b>0.16</b>	35.85	36.60	<b>0.74**</b>
<b>Age Left Education:</b>						
Male Partner	16.64	16.95	<b>0.32**</b>	17.28	17.48	<b>0.21*</b>
Female Partner	16.95	17.62	<b>0.67**</b>	17.41	17.63	<b>0.22*</b>
<b>Number of Children:</b>						
Total	1.93	1.92	<b>-0.01</b>	1.82	1.79	<b>-0.03</b>
Age 0-4	0.62	0.62	<b>-0.00</b>	0.62	0.58	<b>-0.04</b>
Age 5-15	1.31	1.31	<b>-0.01</b>	1.20	1.21	<b>0.01</b>
<b>Housing and Marriage:</b>						
Social Housing	0.29	0.26	<b>-0.04</b>	0.08	0.07	<b>-0.01</b>
Married	0.77	0.75	<b>-0.02</b>	0.86	0.87	<b>0.00</b>
<b>Region:</b>						
North East	0.06	0.05	<b>-0.01</b>	0.05	0.04	<b>-0.01</b>
North West	0.09	0.10	<b>0.01</b>	0.09	0.10	<b>0.01</b>
Merseyside	0.02	0.02	<b>-0.00</b>	0.02	0.02	<b>0.00</b>
Yorkshire and the Humber	0.08	0.10	<b>0.02</b>	0.08	0.08	<b>0.01</b>
East Midlands	0.06	0.07	<b>0.00</b>	0.07	0.08	<b>0.01</b>
West Midlands	0.11	0.09	<b>-0.02</b>	0.10	0.08	<b>-0.01</b>
Eastern	0.07	0.07	<b>0.00</b>	0.09	0.09	<b>0.00</b>
London	0.07	0.07	<b>0.01</b>	0.06	0.07	<b>0.01</b>
South East	0.09	0.09	<b>0.00</b>	0.14	0.14	<b>-0.00</b>
South West	0.06	0.09	<b>0.03</b>	0.09	0.08	<b>-0.00</b>
Wales	0.08	0.06	<b>-0.02</b>	0.05	0.06	<b>0.01</b>
Scotland	0.09	0.09	<b>0.00</b>	0.09	0.07	<b>-0.03*</b>
Northern Ireland	0.10	0.09	<b>-0.01</b>	0.08	0.09	<b>0.01</b>
<b>Labour Market:</b>						
Household Income <sup>4,5</sup>	420.18	446.24	<b>26.06*</b>	606.80	617.87	<b>11.07</b>
Employed (Male)	0.77	0.79	<b>0.02</b>	0.99	0.99	<b>-0.00</b>
Employed (Female)	0.63	0.64	<b>0.01</b>	0.81	0.80	<b>-0.01</b>
Work Hours (Male)	31.38	29.79	<b>-1.59</b>	40.42	39.91	<b>-0.51</b>
Work Hours (Female)	18.81	17.71	<b>-1.10</b>	21.56	20.76	<b>-0.80</b>
<b>Number of Households:</b>	527	725		1052	1453	

Notes:

1. Standard errors(robust) in parentheses. \* p<.10, \*\* p<.05, \*\*\* p<.01
2. Treated group formed of households in the bottom quartile of the year specific net male income distribution
3. Control group formed of households in the inter-quartile range of the year specific net male income distribution
4. £ per week (December 2005 prices)
5. Wages + investment income + social security benefits - taxes

Figure D1: Real Income Growth by Percentile Point (Cribb, Hood, Joyce and Phillips (2013))  
1996–97 to 2011–12



Note: The changes in income at the 1<sup>st</sup>, 2<sup>nd</sup> and 99<sup>th</sup> percentiles are not shown on this graph due to high levels of statistical uncertainty. Incomes have been measured before housing costs have been deducted. Since the distributions of household income in different family types are different, the same percentile points of each distribution do not correspond to the same absolute income levels.

Source: Authors' calculations using Family Expenditure Survey, 1978 to 1980, and Family Resources Survey, 1996–97 and 2011–12.

Table D2: Estimates of the Reform Effect on Durable Ownership

Dependent variable:	Full Sample		High Treatment Intensity	
	(1) Probit mfx	(2) + controls	(3) Probit mfx	(4) + controls
<b><u>Durable Ownership</u></b>				
Mobile Phone	-0.00 (0.023)	-0.02 (0.023)	0.01 (0.024)	0.00 (0.023)
Car	0.02 (0.018)	0.02 (0.016)	0.01 (0.019)	0.02 (0.017)
DVD Player* (N=2930)	-0.03 (0.036)	-0.03 (0.035)	-0.05 (0.040)	-0.04 (0.039)
Computer	-0.01 (0.027)	-0.01 (0.026)	-0.02 (0.029)	-0.02 (0.028)
Internet Access	-0.04 (0.034)	-0.04 (0.034)	-0.05 (0.037)	-0.06 (0.038)
Tumble Dryer	<b>-0.09***</b> (0.033)	<b>-0.09***</b> (0.034)	<b>-0.09**</b> (0.036)	<b>-0.09**</b> (0.037)
Microwave	-0.01 (0.015)	-0.01 (0.014)	-0.00 (0.015)	0.00 (0.013)
Dishwasher	-0.03 (0.037)	-0.03 (0.038)	-0.04 (0.040)	-0.04 (0.042)
Observations	3757	3757	3509	3509

Notes: see table 4 notes.

\* p<.10, \*\* p<.05, \*\*\* p<.01. Standard errors (robust) in parentheses.

Table D3: Working Families Tax Credit maximum amounts (pre-reform)

Tax Credit	Weekly Amount
WFTC (Basic credit)	£61.02
30 hour credit	£11.85
Child credits:	
aged under 16	£26.90
aged 16 - 18	£27.66
Disabled Credits:	
Child	£36.10
Enhanced Child	£47.54
Parent/Couple	£16.53
Childcare tax credit	
1 child	(70% of up to £137.30)
2 children	(70% of up to £203.40)

Table D4: Working and Child Tax Credit maximum amounts (post-reform)

Tax Credit	Weekly Amount
<b>Working Tax Credit (to main earner)</b>	
WTC (Basic credit)	£29.33
Couple element	£28.85
Loan parent	£28.85
30 hour credit	£11.92
Disabled Credits:	
Disability	£39.23
Enhanced disability	£16.63
50 plus:	
Working 16-29 hours	£20.10
Working 30 hours or more	£30.10
Childcare tax credit (paid to carer of children)	
1 child	(70% of up to £135)
2 children	(70% of up to £200)
<b>Child Tax Credit (to carer of children)</b>	
Family element/Basic credit	£10.48
Baby element	£10.48
Child element	£27.79
Disabled Credits:	
Disability	£41.44
Enhanced disability	£16.63

**Source:** Author's calculations based on: Welfare Benefits and Tax Credits Handbook, Child Poverty Action Group, London.

**Notes:** The tables present 2002/03 WFTC rates uprated by the rpi to give hypothetical 2003/04 rates. The new tax credits are based on an annualised calculation using annualised amounts. For ease of comparison, I show weekly equivalents. These are based on the rounding rules in the new tax credits literature for the 2003/04 tax year with 366 days.